Social Nudges Boost Productivity on Online Platforms: Evidence from Field Experiments

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Extending prior work studying how managers can lift productivity, we examine a low-cost, information-based intervention that is conducted by other agents in the work environment. Specifically, we study *social nudges* on online platforms whereby co-users connected with a worker on a platform (or "neighbors") encourage the worker to produce more (content, products, or services). We expect social nudges to boost productivity by conveying neighbors' recognition. To test the effectiveness of social nudges, we conducted a randomized field experiment (N=$1,526,574$) on a video-sharing social network platform where users can act as both content providers and viewers. Treatment providers could receive a message sent by their neighbors encouraging them to produce more videos, whereas control providers could not. Such a simple social nudge boosted video supply by 9.05% and the number of active providers by 10.27% on the receiving day, subsequently increasing the video consumption of these providers' content by 5.56%. Providers who were historically less recognized exhibited larger effects. We also demonstrate that social nudges indeed expand, instead of simply shifting, supply of video content and that the productivity benefits of receiving a social nudge can last five days. Furthermore, leveraging another experiment where treatment providers could receive a message from the platform encouraging them to produce more videos, we provide suggestive evidence that social nudges from neighbors more strongly boost productivity than nudges from the platform. We replicate the main effect of social nudges in a second experiment. Our findings highlight the value of leveraging co-user influence for online platforms.

Key words: Social Nudge, Information-based Interventions, Productivity, Online Platform Operations, Field Experiment

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1. Introduction

Increasing worker productivity has been central to the field of Operations Management (OM) since Frederick Taylor's study of bricklaying manual laborers in the early 20$^{\text{th}}$ century (Winslow 1911). Prior research has focused on what managers (or, broadly speaking, authoritative figures in organizations) can do to lift productivity, since managers in traditional OM settings usually have substantial power to enforce policies and incentives, mandate training, and arrange staffing.

However, beyond managers, the role that other agents within one's work environment (e.g., co-workers, consumers) can play in driving productivity deserves more exploration, especially for online platforms where people provide content, products, or services to digitally connected peers. We study a novel intervention that is initiated by platform users to encourage other users to supply more effort, and we test its short- and long-term effects on productivity via field experiments on an online platform.

Online platforms have had an increasingly large impact on the global economy. For example, seven out of the ten most valuable venture-backed private companies in the *Wall Street Journal* list as of July 2018 were online platforms,[1] and one million EU businesses have already been selling goods and services via online platforms as of December 2019, according to the European Commission.[2] However, compared to traditional firms that hire employees, online platform managers have less control over workers, particularly regarding how often and how much workers produce content, products, or services, and at what quality level (Gurvich et al. 2019, Cachon et al. 2017). Hence, it is valuable to discover effective interventions on online platform workers that are not limited by managers' power.

Building on the well-documented phenomenon that "neighbors" within a social network (e.g., co-workers, classmates, actual neighbors in a physical community) can strongly influence each other (for a review, see Jackson 2010), we believe that a worker's neighbors on a platform—platform users who are connected to this worker—can be a useful but under-exploited source of motivation for the worker. Therefore, we study a type of *social nudge* whereby a worker's neighbors on a platform encourages her to supply more effort on the platform (to produce more content, products, or services).[3] We predict that social nudges initiated by neighbors to encourage production will boost workers' productivity on the platform. Though neighbors may send different forms of social nudges and leverage various persuasion techniques and psychological principles (e.g., social comparison, loss aversion), we believe that one general reason social nudges could work is that by taking the time to nudge a worker, neighbors convey that they value the worker and her prior work. This may make the worker feel more competent and appreciated (Ryan and Deci 2000) and lift her expectations about recognition and even monetary rewards of her future work. Prior psychological and management research suggests that recognition from managers,

[1] https://www.wsj.com/graphics/billion-dollar-club/

[2] https://ec.europa.eu/digital-single-market/en/online-platforms-digital-single-market

[3] The word "nudge" is a Behavioral Science concept for describing interventions that intend to change individuals' behaviors without altering financial incentives or imposing restrictions (Thaler and Sunstein 2009). Nudges are usually implemented by managers, marketers, and policy makers. We coin the term "social nudge" to refer to non-financial, non-restrictive interventions (i.e., nudges) that neighbors implement to influence others in the same network. In this research, we study a type of social nudge that platform neighbors use to influence others' productivity.

companies, or platforms (Bradler et al. 2016, Banya 2017, Ashraf et al. 2014a,b, Gallus 2017) have a positive impact on recipients' productivity and retention. In a similar vein, we expect that by conveying neighbors' appreciation of and interest in nudge recipients' work, social nudges can motivate recipients' productivity.

To causally test the effects of social nudges initiated by neighbors, we conducted two randomized field experiments on a Chinese online video-sharing social network platform (hereafter "Platform O"). Similar to Facebook, each user on Platform O can have two roles: they can upload videos as providers and also watch others' videos as viewers. Users can connect with (hereafter "follow") other users of interest to maintain a closer relationship. In this setting, a user's *neighbors* include other users who follow her and the users that she herself follows. Platform O is more similar to Facebook or Instagram than to YouTube, since it positions itself more as a social network platform, emphasizes short videos capturing users' daily lives, and encourages users to interact with each other like friends.

We studied social nudges sent by one type of neighbor: a user's followers. For all users involved in our experiments, if they had not published videos for one or more days, others who were following them could send them a message to motivate them to upload more videos. To cleanly study the basic effect of being nudged by a platform neighbor, we used a bare-bones, standard message that contained minimal information and only conveyed the neighbor's interest in seeing the recipient' new videos. Users in our experiments were randomly assigned to either the treatment or the control condition. The only difference between users in these two conditions was whether they could actually receive social nudges: Platform O presented treatment users with social nudges sent by their neighbors but blocked social nudges sent to control users. Since the difference between the two groups of users lied in their roles as providers and our primary focus was provider behavior (i.e., content production), we hereafter refer to users involved in our experiments as *providers*.

Our first, main experiment was conducted on September 12th to 14th, 2018 and is the focus of this paper. Our second experiment serves as a replication study and is reported in Online Appendix A. Our analysis involving $1,526,574$ providers reveals several important findings from the main experiment. First, we document a positive short-term effect of social nudges on productivity. Specifically, receiving a social nudge boosted the number of videos uploaded by 9.05% and providers' likelihood of uploading any video by 10.27% on the day of receiving the nudge.[4] Second, we find that providers who were less (vs. more) recognized prior to the experiment boosted their productivity to a greater extent on the day of receiving a social nudge. This finding is consistent with our account that social nudges motivate providers by conveying neighbors' recognition for their work.

[4] Since the median interval between two video uploading incidents across providers in our sample was one day prior to the experiment, we use behavior on the day of receiving a nudge to study the short-term effect of social nudges, and we take a seven-day window to study the long-term effect.

In addition, beyond productivity, we also examine how social nudges affect video consumption. For videos that providers uploaded on the day of receiving a social nudge, treatment providers had 4.29% fewer views per video than control providers. However, this was simply driven by a selection effect: providers who historically had fewer views per video were more likely to upload videos upon receiving a social nudge. More importantly, our intent-to-treat analysis suggests that by prompting treatment providers to upload more videos upon receiving a social nudge, social nudges boosted *total video views* treatment providers brought to the platform by 5.56%, relative to total video views brought by control providers.

Furthermore, we examine the effect of social nudges in a longer time frame. We show that during the week after our experiment, a provider's probability of uploading anything on a given day and the number of videos uploaded each day were still significantly higher in the treatment condition than in the control condition (by 0.88% and 0.80% on average, respectively). This indicates that social nudges expanded supply rather than simply shifting future supply to the present. A more granular analysis suggests that the effect of receiving one social nudge was highest on the day of receiving it, declined with time, but remained significant for five days.

Finally, we analyze a separate field experiment that examines a platform-initiated nudge. A randomly assigned treatment group was sent a standard message from Platform O that encouraged them to upload more videos, while a control group was not sent such a message. We compare the effect of this type of platform-initiated nudge with that of the social nudge observed in our main experiment. We find that the relative effect of receiving a social nudge on providers' uploading likelihood was almost twice as large as that of receiving a platform-initiated nudge on the day of receiving the nudge. Taking a longer time frame, we show that the relative effect of receiving a social nudge on providers' uploading likelihood was more than five times as large as that of receiving a platform-initiated nudge on the first day after receiving the nudge, and around two times as large on the second day after receiving the nudge. Altogether, our findings provide suggestive evidence that nudges from social neighbors may have a stronger effect on productivity than nudges from the platform.

Overall, we propose a low-cost, information-based intervention initiated by platform neighbors, and we test its effects on productivity and consumption on an online platform via large-scale field experiments. Low-cost, information-based interventions like social nudges are valuable and efficient for online platforms. For one, online platforms are particularly eager to control costs, since online platforms mostly create value by facilitating transactions or relationships between users[5] and do not obtain revenue directly from selling products or services. Thus, light-touch, low-cost interventions may be particularly attractive to online platforms. In addition, it is advantageous for online

[5] https://ec.europa.eu/digital-single-market/en/news/commission-staff-working-document-online-platforms

platforms to implement information-based interventions, particularly interventions that utilize co-users' influence, since platforms own advanced information technologies, collect rich information about users' footprints, and digitally connect users.

The rest of the paper is organized as follows. Section 2 reviews the relevant literature and explicates our theoretical contributions. Section 3 develops our hypotheses. In Section 4, we introduce our field setting, experimental design, as well as data and randomization checks. Section 5 presents the short-term effects of social nudges on productivity and consumption. Section 6 presents the long-term effects of social nudges on productivity. In Section 7, we compare the effects of social nudges versus platform-initiated nudges. We discuss implications of our research and directions for future research in Section 8.

2. Literature Review and Theoretical Contributions

Our research primarily builds on four streams of OM literature: productivity, social networks, information-based interventions, as well as platform operations.

First, our research contributes to the OM literature on productivity in service and supply chain settings. Past research has mainly focused on four types of interventions to lift productivity: (1) interventions based on workers' economic considerations such as salary and benefits (Lazear 2000, Celhay et al. 2019), (2) interventions that offer workers training (Konings and Vanormelingen 2015, De Grip and Sauermann 2012) or introduce information technology (Tan and Netessine 2019a), (3) interventions that assign workers to various staffing or workload settings (Tan and Netessine 2014, Moon et al. 2018), and (4) interventions that capitalize on workers' psychological needs and tendencies (Roels and Su 2014, Song et al. 2018). These interventions are usually implemented by firms or managers directly. For example, a firm can change workers' contracts from a fixed hourly rate to a pay-for-performance scheme, or a manager can arrange subordinates' work schedules. Extending prior literature, we study a novel psychology-based intervention that does not originate from firms or managers. Specifically, we study nudges sent by users on an online platform to their neighbors (i.e., co-users with whom they have ties on the platform), and we test the effect of such social nudges in a setting where co-users do not affect each other's financial outcome and have no authority over each other.

Second, our research is related to two streams of research within the social network literature. The first one involves research that empirically assesses social influence within a network (see Jackson 2010 for a review). Research in this area often investigates how schoolmates (Whitmore 2005, Zhang et al. 2017), coworkers (Mas and Moretti 2009, Tan and Netessine 2019b), family members (Dahl et al. 2014, Nicoletti et al. 2018), and residential neighbors and friends (Kuhn et al. 2011, Bapna and Umyarov 2015) affect one's decisions ranging from mundane consumption and product adoption to consequential outcomes about education, health, and career. Drawing insights from this literature, we design and test an intervention that leverages social influence among neighbors within a social network to promote desired behaviors.

The second stream of social-network research we speak to addresses mechanism design issues in social networks. This line of work has largely taken a theoretical perspective to study what the designer of a network can do to achieve desired outcomes, such as maximizing revenue via pricing (Candogan et al. 2012, Cohen and Harsha 2020), maximizing users' brand awareness via advertising (Bimpikis et al. 2016), and maximizing user engagement or minimizing misinformation via information seeding (Zhou and Chen 2016, Candogan and Drakopoulos 2020). In building theoretical models, prior work has often assumed the existence of *positive local network externalities* in a social network—that is, an individual's motivation to take an action increases if her neighbors in the network have taken that action. Extending the existing literature, we design a mechanism for what *neighbors* can do in a social network to help the network achieve desired outcomes, and we empirically study how this mechanism works in an economically meaningful network setting. In addition, by showing that workers produce more on an online platform if their platform neighbors encourage them to do so, our work suggests that neighbors in an online network can exert active influence on each other. This provides empirical support for one channel via which the assumption of positive local network externalities holds in reality.

In addition, our work adds to the emergent OM literature that empirically tests the effectiveness of information-based interventions in solving operational problems. The existing literature has examined interventions such as offering customers more information about firms and the market (Mohan et al. 2020, Xu et al. 2016, Buell and Norton 2011, Parker et al. 2016, Li et al. 2020) as well as offering service providers more information about customers (Buell et al. 2017, Cui et al. 2020, Dai et al. 2020). These information-based interventions have been shown to increase customers' engagement with firms and perceived service value as well as improve service speed and capacity. We contribute to this literature by designing a novel information-based intervention that originates from neighbors within a social network and causally demonstrating its ability to boost productivity.

Finally, our research extends the growing literature that addresses operations problems on online platforms. This literature has examined how to build effective systems for pricing (Zhang et al. 2019a, Bimpikis et al. 2019), recommendations (Banerjee et al. 2016, Mookerjee et al. 2017), and optimization of content production (Caro and Martínez-de Albéniz 2020); it has also studied how to estimate and leverage the spillover effects across platform users (Zhang et al. 2019a,b), as well as how to ensure service quality (Cui et al. 2019, Kabra et al. 2019). In particular, our work is relevant to research that examines service capacity on online platforms, which has analytically studied how to manage service capacity and match supply with demand using different pricing schemes (Cachon et al. 2017, Bai et al. 2019) and staffing rules (Gurvich et al. 2019). Contributing to this research on service capacity and the broader literature on platform operations, we empirically demonstrate that allowing platform users to send social nudges to others, a low-cost and easy-to-implement strategy for any platform, could lift workers' productivity and, in turn, total capacity and consumption on the platform.

3. Hypothesis Development

In this section, we present three hypotheses regarding how social nudges influence workers on online platforms. Our first hypothesis involves the main effect of social nudges on recipients' productivity. We derived our hypothesis from prior research suggesting that people have the general desire to feel valued by others (Ryan and Deci 2000) and that people are willing to exert more effort when they perceive their work is valued by others (Ventrice 2009). In particular, recognition from managers or companies, even without direct material reward, has been shown to improve employees' performances (Grant and Gino 2010, Kosfeld and Neckermann 2011, Bradler et al. 2016, Lourenço 2016). For example, Kosfeld and Neckermann (2011) and Bradler et al. (2016) both found that sending thank-you cards from a manager increased data-entry workers' productivity. Lourenço (2016) showed that sending sales representatives certificates signed by the CEO boosted sales. Grant and Gino (2010) showed that a director of a public U.S. university verbally expressing gratitude made university fundraisers feel more valued and increased the number of calls they made. Similarly, awards granted by managers in traditional organizations or on platforms to recognize workers for their contributions can motivate award recipients, even when they do not involve direct financial or career benefits (Ashraf et al. 2014b, Gallus 2017).

Building on this line of research, we expect social nudges to exert a positive impact on recipients' productivity as well. We note that social nudges may vary in both form and content; sophisticated neighbors may even leverage persuasion techniques and other psychological principles to nudge providers.[6] However, we believe that an important characteristic shared by social nudges, regardless of their specific form and content, is that they convey neighbors' recognition for recipients' work. Neighbors' mere willingness to nudge a worker to produce more conveys that they appreciate her prior work and are interested in her future output. Recognition implied in social nudges, though subtler than recognition expressed by an organization or a representative authority as studied in prior research (Grant and Gino 2010, Kosfeld and Neckermann 2011, Bradler et al. 2016, Lourenço 2016, Ashraf et al. 2014b, Gallus 2017), may be sufficient to motivate recipients to produce more.

[6] Depending on the exact design of a social nudge, its recipient may be affected via additional mechanisms beyond the most basic, generic mechanism we focus on. For example, if a neighbor leverages social norms by telling a worker that many other users have been producing on the same platform, this may affect the worker's productivity via an additional mechanism (e.g., making her feel that supplying effort on the platform is the popular or right thing to do; Cialdini and Goldstein 2004).

Besides feeling recognized for their prior work on a platform, workers who are nudged by neighbors may also feel more optimistic about the attention and recognition their future work will receive, which could further lift their willingness to exert effort on the platform (Stajkovic and Luthans 2003). Moreover, regardless of whether or not workers are directly compensated for contributing effort on an online platform, they are likely to benefit from accumulating a positive reputation on a well-known platform. For example, workers may have better offline employment prospects due to their higher online reputation (Roberts et al. 2006) or may receive better commercial opportunities if they are popular on a platform. Thus, we speculate that as receiving a social nudge informs workers of their neighbors' appreciation for their work and interest in their future efforts, workers may even feel more optimistic about the material or career-related rewards their hard work could bring them in the future. Such positive expectations could also enhance nudge recipients' motivation for providing more content, products or services. Combining all arguments, we formally propose that:

Hypothesis 1 *Receiving a social nudge immediately boosts workers' productivity.*

Next, we propose that the extent to which social nudges boost productivity varies across workers based on their past experiences. While past literature has shown that people typically derive a diminishing marginal utility from financial incentives (Frey and Stutzer 2010), people may also have a diminishing sensitivity to psychological incentives such as social recognition as the result of the general law of diminishing marginal utility (Gossen 1983, Marshall 2009, Horowitz et al. 2007). In this vein, compared to workers whose content/products/services tend to be well recognized, workers whose content/products/services have received little recognition should be more sensitive to and feel more encouraged by getting additional recognition. Combining this perspective with our argument above that social nudges convey neighbors' recognition and can alter workers' expectations for future outcomes, we expect that workers who are historically less (vs. more) recognized will feel more motivated to produce after receiving a social nudge. Formally, we propose that:

Hypothesis 2 *Receiving a social nudge has a stronger impact on workers' productivity when workers are historically less (vs. more) recognized.*

Although we have a clear prediction that social nudges should boost workers' productivity in the short term, the long-term effect of receiving a social nudge is less clear. On one hand, it is possible that workers boost their short-term productivity in response to a social nudge by advancing the content/products/services that they had originally planned to provide in the future. In other words, receiving a social nudge may simply urge workers to shift their supplies to the current period without actually expanding their supplies. Past literature has indeed shown that shifting supply or demand in response to external incentives is common in the supply chain setting. To leverage differences in production costs over time, firms use inventories to shift production across periods (Eichenbaum 1989), retailers often engage in forward buying and build up inventory when manufacturers offer a low price (Blattberg and Neslin 1990), and consumers tend to stockpile and accelerate (rather than expand) their demand at promotional prices, thus purchasing less after sales end (Hendel and Nevo 2004). If indeed short-term benefits brought by social nudges occur at the expense of reduced production afterwards, we would expect workers to produce less during an extended period after receiving a social nudge (compared to the counterfactual case in which they did not receive a social nudge).

On the other hand, it is possible that receiving a social nudge may expand supply and even produce a positive long-term effect. Prior research has shown that the effects of psychological interventions can last for an extended period, even after incentives have been removed or become invisible. For example, Allcott and Rogers (2014) showed that home energy reports that leveraged social norms caused consumers to reduce electricity use immediately and that these efforts, though diminished over time, can last for a few months even after people no longer received such reports. Conrod et al. (2011) found that a personality-targeted intervention continued to reduce alcohol consumption even after the intervention was discontinued. By making providers feel valued and recognized, a social nudge may motivate workers to produce more for an extended period of time. Altogether, we offer the following two competing hypotheses:

Hypothesis 3a *Receiving a social nudge has a negative effect on workers' productivity in the long term.*

Hypothesis 3b *Receiving a social nudge has a positive effect on workers' productivity in the long term.*

4. Experiment Design and Data

In this section, we first describe our field setting and experiment design. Then we provide summary statistics and randomization checks.

4.1. Field Setting and Experiment Design

We collaborated with a Chinese online video-sharing social network platform ("Platform O"). Each user on Platform O can have two roles simultaneously as content providers and viewers. As a content provider, a user uploads videos that are subsequently distributed on the platform, and is free to decide when and what to post; as a viewer, a user watches videos for free. Each user can "follow" other users of interest to maintain closer relationships. The *neighbors* of a user in this setting include both co-users who follow her and those that she herself follows. Compared to popular video-sharing platforms worldwide, Platform O is more similar to Facebook or Instagram than to YouTube in two respects. First, Platform O positions itself more as a social network platform and encourages neighbors to interact and build relationships like friends, similar to Facebook and

Instagram. Second, regarding the length and subjects of videos, Platform O emphasizes short videos that capture users' daily lives, which is again more similar to Facebook and Instagram than to YouTube.

A core challenge faced by Platform O and other online video platforms is encouraging users to produce more videos. We conducted two randomized field experiments to causally test how social nudges from neighbors affect users' video production. Since we are primarily interested in users' behavior as providers (i.e., their content production), we use the term "providers" to refer to the users involved in our experiments. The first experiment lasted from 2PM on September 11, 2018, to 5PM on September 14, 2018. We report this experiment as our main study in the paper. Our second field experiment, a replication of the first, lasted from 5PM on September 14, 2018 to the end of September 20, 2018. Compared to the first experiment, the second experiment targeted a non-overlapping, smaller group of providers but lasted longer. We report data and results about the second (replication) experiment in Online Appendix A.

We studied social nudges sent by one type of neighbor: a provider's followers. During our experiments, users that were following a provider could send a standard message to nudge the provider to upload new videos if the provider had not published videos for one or more days. To do so, followers could simply click a button on the provider's profile page, which read, "Poke this provider" (or "ChuoYiXia" in Chinese; see Figure 1 (a)). We refer to this behavior as "sending a social nudge."

Providers involved in our experiments were randomly assigned to either the treatment or control condition. Treatment providers could view social nudges if they logged onto Platform O after their neighbors had sent them social nudges. Specifically, they would see a red number on top of the "message" button indicating the number of unread messages. By clicking on the "message" button, providers would enter the message center where they could see all messages sent to them, including messages about social nudges. The content of social nudge messages was standard across all providers and read, "[The name of the sender] poked you and wanted to see your new posts" (see Figure 1 (b)).[7] We used such a simple, bare-bones message so as to examine the basic effect of being nudged by a neighbor as cleanly as possible.

If treatment providers did not log onto the platform after social nudges were sent to them, they would not receive any notifications about social nudges. Thus, it is worth noting that (1) treatment providers could not decide whether to visit Platform O based on whether they were sent a social nudge, and (2) social nudges could only affect treatment providers' behavior if treatment providers visited Platform O after neighbors sent them social nudges. For control providers, Platform O

[7] In the message center, more recent messages are placed closer to the top of the screen. Messages about social nudges were not given a higher priority over other types of messages. In general, messages in the message center do not disappear unless the provider deletes them.

specifically blocked all social nudges sent by their neighbors, meaning that they could not receive any social nudges during the experiment *even if their neighbors chose to send them one*. In other words, control providers were unaware of any social nudges that were sent to them. Whether or not providers were able to view social nudges sent to them was the only difference that our experiments manipulated between treatment and control providers. See Figure 2 for an illustration of providers' experiences.

(a) Neighbors Click the Nudge Button on Providers' Profile Page

(b) Example of A Notification about A Social Nudge

Figure 1 Illustration of how social nudges are sent by neighbors and displayed to treatment providers [8]

4.2. Data and Randomization Checks for the Main Experiment

Our sample of providers included treatment and control providers who satisfied two criteria: (1) at least one of their neighbors sent them a social nudge during our experimental period, and (2) they logged onto Platform O at least once during our experimental period on or after the day when the first social nudge for them was sent (so they could see social nudges sent to them, had they been randomly assigned to the treatment condition). For example, if a provider's first social nudge was sent at 6PM on September 12, 2018, then the provider was included in our sample as long as she visited Platform O during the period of September 12-14, 2018; if a provider's first social nudge was sent at 8AM on September 13, 2018, then the provider was included in our

[8] To protect Platform O's identity, we created Figure 1 by taking the app interface of a widely-used video-sharing platform in China and digitally altering some non-essential details to reflect where the nudge button and social nudge message are and what they look like on Platform O. Platform O has a similar app interface.

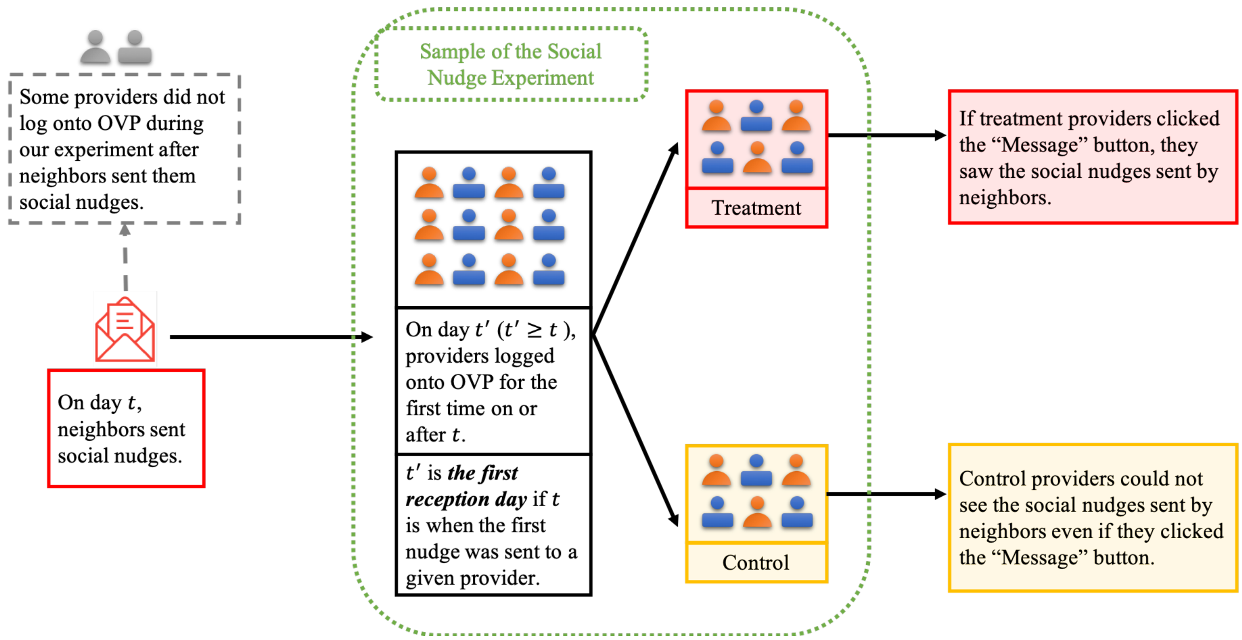


Figure 2 **Illustration of the experience of treatment versus control providers**

sample as long as she visited Platform O on September 13 or 14. Our sample included $1,526,574$ qualified providers, with $763,163$ assigned to the treatment condition and $763,411$ assigned to the control condition. Treatment and control providers in our sample preserved the benefits of random assignment because our random assignment of providers into the treatment vs. control condition could not affect (1) when their neighbors sent the first social nudge during the experiment or (2) whether or when providers logged onto Platform O after the first social nudge was sent to them.

For both treatment and control providers in our sample, we defined "the first reception day" as the day that they first visited Platform O on or after the day the first social nudge for them was sent. For example, if a provider's first social nudge was sent to her on September 12, 2018 and she visited Platform O on September 12, 2018, then we took September 12, 2018 as her first reception day; if the provider did not visit Platform O on September 12 but visited on September 13, then we took September 13 as her first reception day. Note that while treatment providers were able to see social nudges on their first reception day (even though they might not have entered the "message" center and seen the social nudges that day), control providers were not able to. See Figure 2 for an illustration of our sample selection criteria and the identification of the first reception day.

To confirm the success of randomization among our sample of providers, we compared treatment providers and control providers in their gender, the number of users who followed them, and their pre-experiment production performance statistics. Our randomization check results are reported in Table 1, where the proportion of females reflects raw data and the other variables are standardized to have zero mean and unit standard deviation to protect sensitive data. Table 1 shows that

Table 1 Randomization Check

		Treatment providers (1)	Control providers (2)	p-value of t-test (3)
Statistics on the day prior to the Experiment	Proportion of Females	54.22%	54.16%	0.46
	Number of Followers	4.23×10^{-3}	-4.23×10^{-3}	0.60
Statistics during one week prior to the Experiment	Number of Uploaded Videos	5.99×10^{-3}	-5.99×10^{-3}	0.46
	Number of Days with Videos Uploaded	5.89×10^{-3}	-5.89×10^{-3}	0.47
Statistics since January 1, 2018 to the day prior to the Experiment	Historical Average Views per Video	-2.95×10^{-3}	2.95×10^{-3}	0.73
Statistics during the Experiment	Distance between the Day When the First Nudge Was Sent and the First Reception Day	6.53×10^{-3}	-6.53×10^{-3}	0.42

Note: All variables, other than the proportion of females, were standardized to have zero mean and unit standard deviation.

Note: When calculating the proportion of females, we excluded the 8,880 providers (0.6%) who did not have gender information.

Note: Due to the nature of historical average views per video, this variable does not have values for providers who did not upload any video between January 1, 2018 and September 11, 2018, including 92,612 (6.1%) users who newly became providers during our experimental period (i.e., they were not providers on and before September 11, 2018) and 33,586 (2.2%) providers who became providers before September 11, 2018 but did not upload any videos between January 1, 2018 and September 11, 2018.

treatment and control providers in our sample were similar in the proportion of female providers, the number of followers on the day prior to the experiment, the number of videos they uploaded and the number of days when they uploaded any video during the one week prior to the experiment, as well as their historical average views per video (i.e., the total number of views a provider received from January 1, 2018 to the day before the experiment, divided by the number of videos she uploaded during that same period). In addition, for each provider, we calculated the distance (in days) between the day when the first nudge was sent to her and her first reception day. For example, if the first nudge was sent to her on September 12, 2018, and she logged onto Platform O on September 13, 2018, the distance was one day. We confirmed that the distance was virtually the same between treatment and control providers, as expected from random assignment. These results confirmed that the treatment and control providers in our sample are comparable, suggesting that any difference between conditions after the experiment started should be attributed to whether providers could receive social nudges.

5. The Short-Term Effects of Social Nudges

We began our investigation by examining the short-term effects of social nudges on providers' productivity and the video consumption they bring to the platform.

5.1. The Short-term Effects of Social Nudges on Productivity

We first tested whether social nudges have a positive effect on productivity in the short term (Hypothesis 1). From January 1, 2018 to the day prior to the experiment, the median interval between two successive incidents of video uploading across providers in our sample was one day. Considering this frequency, it was possible for providers to upload a new video on the same day that they received a social nudge, and it is reasonable to denote the same day providers received a social nudge as the "short term." Moreover, most (82%) providers were sent only *one* social nudge during our experiment. Hence, to test Hypothesis 1, we examined each provider's production on the first reception day—that is, the day she first visited Platform O on or after the day she was sent the first social nudge.[9] Our unit of analysis was a provider on her first reception day, and our analysis involved $1,526,574$ observations, with each provider contributing one observation.

We used the following ordinary least squares (OLS) regression specification to test the causal effects of social nudges in the short term:

$$Outcome\ Variable_i = \alpha_0 + \alpha_1 Treatment_i + \epsilon_i \tag{1}$$

where $Outcome\ Variable_i$ is detailed later, and $Treatment_i$ is a binary variable indicating whether provider i is in the treatment (vs. control) condition. To protect Platform O's data privacy, we standardized all continuous variables used in our regression analyses (including outcome variables and predictor variables) to have zero mean and unit standard deviation. To facilitate interpretations of the magnitude of each observed main effect, we also report its relative effect size, which we obtained by running the same regression on raw data without standardization and dividing the estimated average treatment effect (i.e., the coefficient on treatment) by the average of the corresponding outcome variable in the control condition.

For each provider i, we first examined the number of videos she uploaded on the first reception day ($Number\ of\ Videos\ Uploaded_i$), which equals 0 if provider i didn't upload any videos that day. We used regression specification (1) to predict $Number\ of\ Videos\ Uploaded_i$ and report the

[9] Since we only had access to providers' login dates but not specific login times, we could not identify which providers logged onto Platform O at a time of day after the first social nudge was sent to them. Note that among providers who visited Platform O on the day their first social nudge was sent, some may visit only *before* the nudge was actually sent. For these providers, we took that day as their first reception day and assumed that they could see the social nudge that day (if they were in the treatment condition), even if they could not in reality (since they did not visit Platform O that day after the nudge was sent). As a result, this approach gives us a conservative estimate of the short-term effect of receiving a social nudge.

Table 2 Short-term Effects of Social Nudges on Productivity

Panel A: Main Effect on Productivity			
Outcome variable	Number of Videos Uploaded	Upload Incidence	Daily Capacity
	(1)	(2)	(3)
Treatment	0.0261****	0.0136****	-0.0134**
	(0.0016)	(0.0006)	(0.0043)
Relative effect size	9.06%	10.27%	-1.10%
Observations	1,526,574	1,526,574	212,356

Panel B: Testing the Role of Historical Daily Capacity: Evidence for the Selection Effect		
Outcome variable	Historical Daily Capacity	Upload Incidence
	(1)	(2)
Treatment	-0.0240***	0.0186****
	(0.0061)	(0.0010)
Historical Daily Capacity		0.0602****
		(0.0007)
Treatment * Historical Daily Capacity		-0.0041***
		(0.0010)
Relative effect size	-1.35%	
Observations	163,241	693,770

Note: Standard errors are reported in the parentheses. *p<0.05; **p<0.01; ***p<0.001; ****p<0.0001

Note: Number of videos uploaded, daily capacity, and historical daily capacity were standardized to have zero mean and unit standard deviation before entering the regressions.

Note: The unit of analysis for all columns in Panels A and B was a provider on her first reception day. Columns (1)-(2) in Panel A included all providers in our sample. Column (3) in Panel A included the subset of providers who uploaded something on her first reception day. Column (1) in Panel B included the subset of providers who uploaded something on her first reception day and uploaded at least a video during one week prior to our experiment. Column (2) in Panel B included the subset of providers who uploaded at least one video during the week prior to our experiment.

regression results in Column (1) of Table 2. The positive and significant coefficient on treatment indicates that receiving a social nudge had a positive, short-term effect on productivity. Specifically, receiving a social nudge increased the number of videos uploaded on the first reception day by 0.0261 standard deviations ($p < 0.0001$), which amounts to a 9.06% increase relative to the average in the control condition.[10]

The boost in the number of videos uploaded on the first reception day may have been driven by two forces: (1) providers became more willing to upload at least one video on the first reception day, and (2) providers who decided to upload at least one video on the first reception day uploaded more videos on that same day. To test the presence of the first force, for each provider i, we examined

[10] The short-term effect of social nudges is mostly driven by receiving *one* social nudge (rather than multiple social nudges on one day) since, for 90% providers, only *one* social nudge was sent them by the first reception day.

whether or not she uploaded at least one video on the first reception day ($Upload\ Incidence_i$). To test the presence of the second force, we examined the number of videos uploaded on the first reception day among providers who uploaded at least one video that day ($Daily\ Capacity_i$).

We used regression specification (1) to predict $Upload\ Incidence_i$ and $Daily\ Capacity_i$. As shown in Column (2) of Table 2, receiving a social nudge lifted the average probability of providers uploading any videos on the first reception day by 1.36 percentage points ($p < 0.0001$). This is a 10.27% increase relative to the average probability in the control condition. However, as shown in Column (3) of Table 2, the negative coefficient on treatment indicates that $Daily\ Capacity_i$ was lower in the treatment condition than in the control condition. Specifically, among the providers who uploaded anything on the first reception day, treatment providers uploaded 1.10% fewer videos than control providers, a difference of 0.0134 standard deviations ($p < 0.01$). These results suggest that the immediate, positive impact of social nudges on productivity is driven by a boost in providers' willingness to produce any content (i.e., the extensive margin), not by a boost in the number of videos uploaded after they have already uploaded at least one video (i.e., the intensive margin). If anything, we find that, conditional on uploading any content, providers' daily capacity was slightly lower in the treatment condition than in the control condition.

This slight decrease in the providers' daily capacity on the first reception day may have occurred as a result of the *selection effect* of social nudges. That is, providers who historically tended to upload fewer (vs. more) videos a day may be more likely to upload something after they received a social nudge. This, if true, would mean that treatment providers who uploaded something on the first reception day consisted of more providers who tended to upload a small number of videos a day (i.e., having a smaller daily capacity), compared to control providers who uploaded something on the first reception day.

To provide evidence on the existence of such selection effects, we calculated $Historical\ Daily\ Capacity_i$, which equals the average number of videos provider i uploaded per day across days when she uploaded at least one video during the week prior to the experiment. We conducted two analyses using this measurement. First, we examined whether treatment providers (vs. control providers) who uploaded anything on the first reception day had a smaller historical daily capacity. We used regression specification (1) but replaced the outcome variable with $Historical\ Daily\ Capacity_i$ and focused on providers who uploaded something on the first reception day and uploaded at least one video during the week prior to the experiment (thus having a value for $Historical\ Daily\ Capacity_i$). Indeed, among these providers, those in the treatment condition historically tended to upload 1.35% fewer videos on days when they uploaded any content, compared to those in the control condition ($p < 0.001$; Column (1) of Table 2 Panel B). In other words, treatment providers (vs. control providers) who uploaded anything on the first reception day tended to have a lower daily capacity *before* the experiment, which provides the initial evidence that the selection effect exists.

Second, we examined whether social nudges were more likely to prompt providers with a smaller historical daily capacity to upload videos. We used the following regression specification to test the interaction between the social nudge treatment and providers' historical daily capacity in predicting upload incidence on the first reception day (where we standardize $\mathit{Historical\ Daily\ Capacity}_i$ to have a mean of zero and unit deviation):

$$\begin{aligned} \mathit{Upload\ Incidence}_i = & \alpha_0 + \alpha_1 \mathit{Treatment}_i + \alpha_2 \mathit{Historical\ Daily\ Capacity}_i \\ & + \alpha_3 \mathit{Treatment}_i * \mathit{Historical\ Daily\ Capacity}_i + \epsilon_i \end{aligned} \tag{2}$$

We present results from specification (2) in Column (2) of Table 2 Panel B. This regression only included providers who uploaded at least one video during the week prior to the experiment and thus had a value for $\mathit{Historical\ Daily\ Capacity}_i$. The negative coefficient on the interaction term indicates that the lower a provider's historical daily capacity, the larger the effect of receiving a social nudge on her upload incidence ($p < 0.001$). In other words, providers with a lower daily capacity prior to the experiment increased their probability of uploading any content in response to social nudges to a greater extent than providers with a higher daily capacity did. This provides further evidence for the existence of the selection effect.

5.2. The Heterogeneous Treatment Effect of Social Nudges based on Providers' Historical Recognition

We next tested how providers' historical recognition affects their short-term response to social nudges. We predicted that less recognized (vs. more recognized) providers would experience a greater sense of encouragement from social nudges and thus boost their productivity to a greater extent (Hypothesis 2). We proxied each provider i's historical recognition by $\mathit{Historical\ Average\ Views\ per\ Video}_i$. This variable equals the total number of views provider i received from January 1, 2018 to the day prior to the experiment, divided by the number of videos that provider i uploaded during that same period. By construction, our analyses involving this variable only included providers who uploaded at least a video between January 1, 2018 and September 11, 2018 (see the note on Table 1 for more detail). To deal with outliers in $\mathit{Historical\ Average\ Views\ per\ Video}_i$ due to a small number of videos going viral, we winsorized the top 1% of data points for $\mathit{Historical\ Average\ Views\ per\ Video}_i$.[11] That is, we replaced values of

[11] Winsorizing is a valid and popular tool for dealing with outliers (Reifman and Keyton 2010), named after the biostatistician Charles P. Winsor, and has often been used in empirical research (e.g., Campbell et al. 2011, Dewan et al. 2007). The result of this analysis as well as other results presented in the paper are robust if we log-transform dependent and predictor variables that are continuous. For continuous variables that contain zero, our results are robust when we add different small numbers to the raw data before conducting log-transformation.

Table 3 Heterogeneous Treatment Effect based on Historical Recognition

Outcome variable	Upload Incidence		
Sample	Providers Having Historical Average Views per Video	Providers with Historical Average Views per Video below Median	Providers with Historical Average Views per Video above or equaling median
	(1)	(2)	(3)
Treatment	0.0146****	0.0162****	0.0130****
	(0.0006)	(0.0008)	(0.0009)
Historical Average Views per Video	-0.0033 ****		
	(0.0004)		
Treatment * Historical Average Views per Video	-0.0026 ****		
	(0.0006)		
Observations	1,400,376	698,802	701,574

Note: Standard errors are reported in the parentheses. *p<0.05; **p<0.01; ***p<0.001; ****p<0.0001

Note: The variable, historical average views per video, was standardized to have zero mean and unit standard deviation before entering the regression in Column (1).

Note: The unit of analysis for all columns was a provider on her first reception day. Column (1) included all providers who uploaded at least one video between January 1, 2018 and the day prior to the experiment and thus had values for *Historical Average Views per Video*. Column (2) included providers whose *Historical Average Views per Video* was below the median of this variable, while Column (3) included providers whose *Historical Average Views per Video* was above or equaled the median of this variable.

$\textit{Historical Average Views per Video}_i$ that were greater than the 99^{th} percentile of this variable with the 99^{th} percentile of this variable. Our results are robust if we winsorize at the 95^{th} percentile. After winsorization, the variable $\textit{Historical Average Views per Video}_i$ was then standardized to have a mean of zero and unit deviation. As for the outcome variable, we focused on $\textit{Upload Incidence}_i$ to test Hypothesis 2 since Section 5.1 suggests that the key effect of social nudge on productivity reflects a boost in providers' likelihood of uploading a video (i.e., the extensive margin). The results are robust if we use $\textit{Number of Videos Uploaded}_i$ as the outcome variable.

First, we used the following regression specification to test whether social nudges caused providers with lower $\textit{Historical Average Views per Video}_i$ to exhibit a larger boost in their probability of uploading any video on the first reception day:

$$\begin{aligned}\textit{Upload Incidence}_i =& \alpha_0 + \alpha_1 \textit{Treatment}_i + \alpha_2 \textit{Historical Average Views per Video}_i \\ &+ \alpha_3 \textit{Treatment}_i * \textit{Historical Average Views per Video}_i + \epsilon_i\end{aligned} \tag{3}$$

As shown in Table 3 Column (1), the coefficient on the interaction between $\textit{Treatment}_i$ and $\textit{Historical Average Views per Video}_i$ is significant and negative ($p < 0.0001$). This suggests that social nudges exhibited a stronger short-term effect on providers' willingness to upload a video if providers had a lower historical recognition.

Next, to facilitate interpretations of this heterogeneous treatment effect, we split our sample into two sub-samples based on whether $\textit{Historical Average Views per Video}_i$ was below the median or not. For each sub-sample, we separately estimated the effect of social nudges on $\textit{Upload Incidence}_i$ using specification (1). According to Table 3, receiving a social nudge significantly boosted upload incidence in both sub-samples (both p-values < 0.0001), but providers whose *Historical Average Views per Video* was below the median exhibited a larger effect (1.62 percentage points in Column (2)), compared to providers above or equaling the median (1.30 percentage points in Column (3)). Altogether, these results support Hypothesis 2 that less recognized (vs. more recognized) providers boost their productivity to a greater extent after receiving a social nudge.

5.3. The Short-Term Effects of Social Nudges on Consumption

Beyond productivity, it is also worth examining how social nudges affect video consumption. One natural approach is to simply look at videos that were uploaded by treatment and control providers and compare the number of views they received. We began with this simple approach and discuss its limitations later. Following Platform O's common practice, for each video uploaded on a provider's first reception day, we tracked the number of views it accumulated over the week since its creation. Platform O normally looks at the views each video accumulates during the first week to capture the short-term consumption it brings, considering that videos on Platform O are usually watched more frequently during the first week after its creation and attract fewer views as time goes by. For provider i who uploaded any video on the first reception day, the average number of views per video ($\textit{Average Views per Video}_i$) equals the average of the number of one-week views across all videos provider i uploaded on the first reception day. This variable does not have values for providers who did not upload any videos on the first reception day. Similar to how we dealt with extreme outliers for $\textit{Historical Average Views per Video}_i$ (Section 5.2), we winsorized the top 1% of data points for $\textit{Average Views per Video}_i$. Our results are robust if we winsorize at the 95$^{\text{th}}$ percentile.

We used regression specification (1) to predict $\textit{Average Views per Video}_i$. As shown in Column 1 of Table 4, the average number of views for videos uploaded on the first reception day was significantly lower among treatment providers than among control providers by 0.0155 standard deviations, which amounts to a 4.29% decrease relative to the average in the control condition. Such a difference may appear to suggest that social nudges reduce video consumption. However, two critical aspects of the relationship between social nudges and video consumption must first be taken into account.

First, the aforementioned finding about average views per video could have been driven by a *selection effect*, not a *treatment effect*. As shown in Section 5.2, social nudges exhibited a stronger short-term effect on providers with a lower historical recognition (i.e., providers whose videos were

Table 4 Short-term Effects of Social Nudges on Video Consumption

Outcome variable	Average Views per Video	Historical Average Views per Video	Average Views per Video	Total Views
	(1)	(2)	(3)	(4)
Treatment	-0.0155***	-0.0163***	-0.0049	0.0068****
	(0.0043)	(0.0044)	(0.0034)	(0.0016)
Historical Average Views per Video			0.6199****	
			(0.0017)	
Observations	212,356	209,490	209,490	1,526,574
Relative effect size	-4.29%	-6.23%		5.56%

Note: Standard errors are reported in the parentheses. *p<0.05; **p<0.01; ***p<0.001; ****p<0.0001

Note: Average views per video, historical average views per video, and total views were standardized to have zero mean and unit standard deviation before entering the regressions.

Note: The unit of analysis for all columns was at the provider level. Column (1) included the subset of providers who uploaded something on their first reception day. Columns (2)-(3) included the subset of providers who uploaded something on their first reception day and uploaded at least one video between January 1, 2018 and the day prior to the experiment (September 11, 2018). Column (4) included all providers in our sample.

historically viewed fewer times). As a result, among providers who uploaded anything on the first reception day, those in the treatment condition should have had lower historical average views per video than those in the control condition. Indeed, as shown in Table 4 Column (2), among these who uploaded videos on the first reception day, treatment providers' historical average views were lower than control providers' by 0.0163 standard deviations (or 6.23%). Such a selection effect could lead the average views for videos uploaded on the first reception day to be lower in the treatment condition than in the control condition. In fact, when we predicted $\textit{Average Views per Video}_i$ while controlling for $\textit{Historical Average Views per Video}_i$, the coefficient on treatment was no longer significant (Column (3) in Table 4).[12] This suggests that the decrease in average views per video after providers received social nudges was driven by the *selection effect*, whereby providers with a low historical recognition were more likely to upload videos upon receiving a social nudge, and not by the *treatment effect*, whereby social nudges caused providers to produce less attractive videos.

Second, although treatment providers who uploaded any video on the first reception day had fewer average views per video than their control provider counterparts (due to the selection effect mentioned above), social nudges may still have had a positive impact on the overall video consumption of a provider's content. In other words, the total number of views a provider brings during a

[12] Column (3) in Table 4 did not include the small number of providers who did not have a value for *Historical Average Views per Video*. If we run the same regression as that reported in Column (1) but exclude providers without a value for *Historical Average Views per Video*, the coefficient of treatment is still significantly negative and basically remains unchanged from the coefficient reported in Column (1). Hence, the fact that the coefficient on treatment becomes insignificant in Column (3) after we control for $\textit{Historical Average Views per Video}_i$ is not simply due to the loss of observations about providers without a value for *Historical Average Views per Video*.

given time period may be lifted by the treatment, since social nudges do have a substantial effect on the number of videos providers upload (as shown in Section 5.1). To evaluate the short-term effect of social nudges on overall video consumption brought by a provider, we examined the total number of views each provider engendered that could be attributed to videos they uploaded on the first reception day. Specifically, for each provider i, $\textit{Total Views}_i$ equals the total number of views across all videos that provider i uploaded on the first reception day. If provider i did not upload videos on the first reception day, $\textit{Total Views}_i$ equals zero, which reflects the fact that no views were brought by provider i as a result of her production effort on the first reception day. We only considered the views each video accumulated during the week after they were uploaded, for the reason explained earlier. Similar to how we addressed outliers for $\textit{Average Number of Views}_i$ and $\textit{Historical Average Views per Video}_i$, we winsorized $\textit{Total Views}_i$ at the 99^{th} percentile of non-zero values.[13]

We used regression specification (1) to predict $\textit{Total Views}_i$. As shown in Column (4) of Table 4, the positive and significant coefficient on treatment indicates that social nudges indeed boosted the total number of views providers contributed to the platform as a result of their production effort on the first reception day ($p < 0.0001$). Specifically, receiving a social nudge increased total views—a more important metric for the platform than $\textit{Average Views per Video}_i$—by 0.0068 standard deviations, which amounts to a 5.56% increase relative to the average in the control condition.

6. The Long-term Effects of Social Nudges on Productivity

So far, we have shown that social nudges can significantly lift providers' willingness to upload videos on the first reception day and in turn lead them to contribute more views to the platform in the coming week. However, since the median interval between two video uploading incidents prior to the experiment was *one day* for providers in our sample, it is possible that treatment providers simply uploaded some of the videos that they were originally planning to upload a couple of days later and shifted their supply forward to the day they received a social nudge, in response to the social nudge. In this case, the immediate boost of videos uploaded may come at the cost of reduced future productivity, which would be undesirable for Platform O. To address this issue, we examined the long-term effects of social nudges (Hypothesis 3). Given the high frequency of video uploading behavior among providers in our sample, we focused on one week in our long-term analysis (in contrast to the one-day window for our short-term analysis).

[13] Since the majority of providers did not produce any videos on the first reception day and consequently had a value of zero for $\textit{Total Views}_i$, the 99^{th} percentile of the raw values of $\textit{Total Views}_i$ was small. Since we wanted to address extreme outliers caused by a small number of videos that went viral, we winsorized at the 99^{th} percentile of non-zero values. That is, we replaced values of $\textit{Total Views}_i$ that were greater than the 99^{th} percentile of the non-zero values with the 99^{th} percentile of the non-zero values.

We took two approaches to assess how social nudges affect productivity in the long term. First, we explored how productivity differed between the two conditions after the experiment ended, at which point both treatment and control providers could receive social nudges sent to them and thus experienced no differential treatment. A lower productivity in the treatment (vs. control) condition after the experiment would be concerning, because it would suggest that the increased productivity among treatment providers during the experiment may come from treatment providers accelerating the timing of uploading videos rather than expanding their supply (Hypothesis 3a). In contrast, a higher productivity in the treatment condition (Hypothesis 3b) or the lack of a significant difference between two conditions would suggest that our social nudge treatment actually expanded supply. Second, we explored how the effect of receiving a social nudge changed over time among providers who only received one nudge during the entire experimental period. Since these providers were essentially unaffected by the experiment beyond their first reception day, this analysis provides more information on how long the effect of a single social nudge can last.

6.1. The Effects of Social Nudges on Productivity During One Week After the Experiment

To examine how our social nudge treatment affected productivity after the end of the experiment, we focused on September 15, 2018 - September 21, 2018 (the week after the experiment). Our unit of analysis was at the provider-day level such that each provider's productivity each day was one observation. We used the following OLS regression specification to test the causal effects of social nudges after the experiment ended:

$$Outcome\ \ Variable_{it} = \alpha_0 + \alpha_1\, Treatment_i + \epsilon_{it} \tag{4}$$

where $Outcome\ Variable_{it}$ includes $Upload\ Incidence_{it}$ — whether provider i uploaded at least a video on day t, and $Number\ of\ Videos\ Uploaded_{it}$ — the total number of videos uploaded by provider i on day t (which equals 0 if $Upload\ Incidence_{it}$ is 0), and t ranges from September 15 to September 21. Standard errors are clustered at the provider level.

According to Table 5 Columns (1) and (2), even during the week after our experiment was terminated, providers' probability of uploading anything on a given day was still significantly higher in the treatment condition than in the control condition ($p < 0.0001$). The average difference of 0.14 percentage points per day amounts to a 0.88% increase compared to the average probability in the control condition. Similarly, the number of videos uploaded on a day was higher in the treatment condition than in the control condition ($p < 0.0001$). The average difference of 0.0025 standard deviations amounts to a 0.80% increase compared to the average in the control condition ($p < 0.0001$). These results suggest that treatment providers did not produce less than control providers after the experiment ended; in fact, treatment providers still uploaded more videos than control

Table 5 The Effects of Social Nudges on Productivity During One Week After the Experiment

	Time Window: September 15, 2018 - September 21, 2018	
Outcome variable	Upload Incidence	Number of Videos Uploaded
	(1)	(2)
Treatment	0.0014****	0.0025****
	(0.0004)	(0.0010)
Relative effect size	0.88%	0.80%
Observations	10,686,018	10,686,018

Note: Standard errors are clustered at the provider level and reported in the parentheses. $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$; $^{****}p<0.0001$

Note: Number of videos uploaded was standardized to have zero mean and unit standard deviation before entering the regression in Column (2).

Note: The unit of analysis for both columns was at the provider-day level, and both columns included all providers in our sample.

providers during the week after the experiment, although the effect size there was understandably smaller than that during the experimental period. This positive effect of our treatment on productivity during the week after our experiment supports Hypothesis 3b and suggests that social nudges indeed encourage providers to expand their supply.

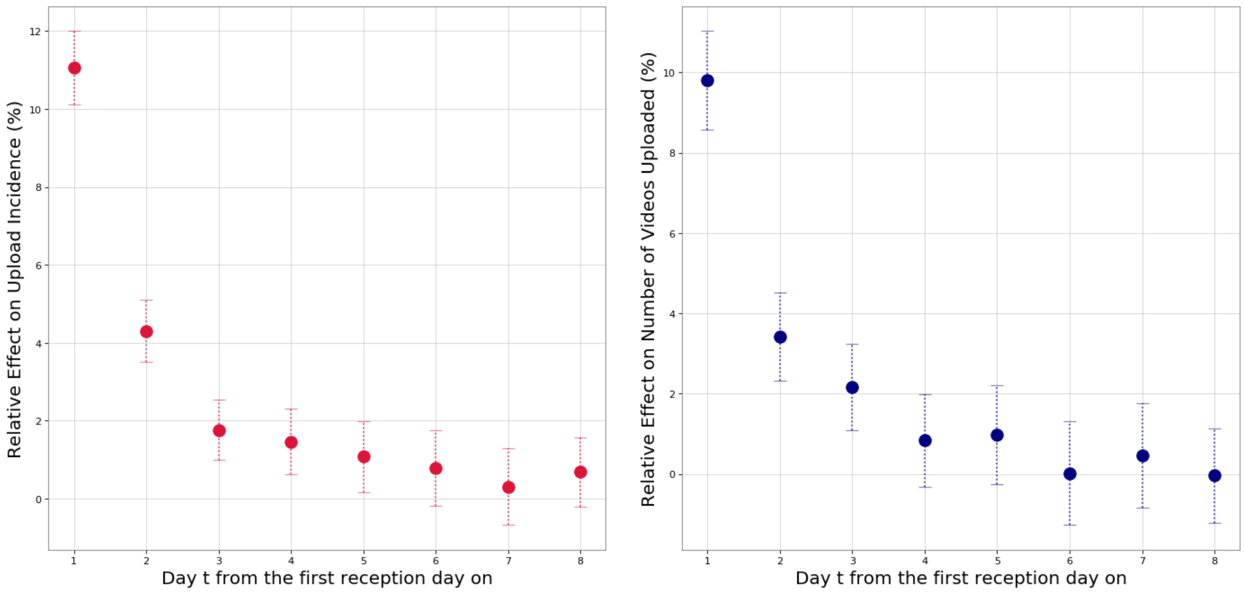
6.2. The Over-Time Effect of Receiving Social Nudges on Productivity

To examine how the effect of receiving *a single* social nudge on productivity changes over time, we next focused on providers who were only sent one social nudge during our experiment (i.e., 82% of the sample)[14] and assessed productivity differences between the two conditions each day up to a week following the first reception day. Specifically, for each day t starting from the first reception day (where t equals $1, 2, \cdots, 8$ and $\mathrm{t} = 1$ refers to the first reception day itself), we predicted $Upload\ Incidence_{it}$ and $Number\ of\ Videos\ Uploaded_{it}$ using regression specification (1). Each day had $1,256,079$ observations corresponding to the number of providers who were sent only one social nudge during our experiment. For comparability between two outcome variables, we plotted the relative effect sizes across days in Figure 3.[15] We obtained the relative effect size each day by applying regression specification (1) to raw data without standardization and dividing the coefficient on treatment on a day for a given outcome variable by the average of that outcome variable in the control condition that day. The corresponding 95% confidence intervals equaled the

[14] We note that this sample is not perfect for a causal estimation of the effect of receiving a single nudge (see the detailed reason in Online Appendix C), but this is the best possible approach. Also, as a robustness check, we conducted the same analysis for the full sample of providers using information about their first social nudge, which showed very similar results as our analysis about providers who were sent only one social nudge during our experiment (see Online Appendix C).

[15] We report the absolute effect sizes and the corresponding 95% confidence intervals in Online Appendix B.

upper and lower bounds of the coefficient on treatment for a given outcome variable divided by the average of that outcome variable in the control condition.



(a) Over-time Effect on Upload Incidence (b) Over-time Effect on Number of Videos Uploaded

Note: The error bars represent 95% confidence intervals.

Figure 3 Illustration of how the relative effect size of receiving a social nudge varied over time

As shown in Figure 3 (a), the relative effect of receiving a social nudge on $Upload\ Incidence_{it}$ was largest on the first reception day (i.e., $t = 1$), decreased as time elapsed, but was always in the positive direction. In fact, treatment providers' likelihood of uploading any video on the day after the first reception day (i.e., $t = 2$) was higher than that of control providers by 4.31% ($p < 0.0001$), and the positive effect stayed above a 1% relative effect size and remained statistically significant for another three days (from $t = 3$ to $t = 5$). As shown in Figure 3(b), the relative effect of receiving a social nudge on $Number\ of\ Videos\ Uploaded_{it}$ decreased with t and remained significantly positive for two more days after the first reception day.[16]

To sum up, after receiving a social nudge, treatment providers exhibited a higher willingness to upload videos than control providers for a period of time (five days including the first reception day). That the positive effect of receiving a social nudge decreases with time is not surprising since social nudges on Platform O are displayed privately to providers in a relatively hidden place and

[16] The effect on $Number\ of\ Videos\ Uploaded_{it}$ remained statistically significant for two fewer days than the effect on $Upload\ Incidence_{it}$, which may be explained by (1) receiving a social nudge having a smaller effect for $Number\ of\ Videos\ Uploaded_{it}$ than for $Upload\ Incidence_{it}$ (due to the selection effect explained in Section 5.1) or (2) a higher variance of $Number\ of\ Videos\ Uploaded_{it}$ than $Upload\ Incidence_{it}$.

the salience of a social nudge naturally would fade as time elapses (especially as more messages arrive at the message center and bury messages about social nudges). It is also important to note that Figure 3 illustrates the effect of receiving *one* social nudge (since we only considered providers who were sent a single social nudge during our experiment for the analyses presented in this figure). The positive effect of enabling viewers to nudge providers will not fade completely, since neighbors can continue to send social nudges if providers do not upload any video for a while. Successive social nudges from neighbors may help push providers to maintain activity in the long term.

7. Comparing Social Nudges with Platform-Initiated Nudges

As we hypothesized in Section 3, social nudges sent by platform neighbors convey neighbors' recognition. Receiving a social nudge and its implied recognition may make providers feel more valued and more optimistic about their future videos' success, thus motivating them to produce new videos. However, such information communicated via social nudges from neighbors may not be passed on by nudges from the platform to encourage production. To explore whether social nudges have effects beyond regular nudges sent from companies, we examined another randomized field experiment that tested the effects of receiving nudges from Platform O. We refer to this experiment as the *platform-initiated nudge experiment*. We note that comparing the results of our main social nudge experiment versus the platform-initiated nudge experiment does not causally estimate the difference between social nudges and platform-initiated nudges, since the two experiments were conducted in different time periods and providers were not randomly assigned to receive one of these two types of nudges. Thus, the providers included in the two experiments may not be exactly comparable. As described below, we strove to construct samples from the two experiments that were as comparable to each other as possible. We acknowledge the limitation of comparing results across two experiments and encourage future research to causally and systematically compare these two types of nudges in General Discussion.

7.1. Experiment Design and Data

The platform-initiated nudge experiment was conducted between 9AM on July 22, 2019 and 5AM on August 30, 2019. Half of the providers were randomly assigned to the treatment condition, and the other half to the control condition. During the experiment, the platform identified providers who published a video one or more years ago exactly on the same date. For these providers, Platform O created a message that read, "On this day X years ago, you posted a video. Post another one to capture the moments today!" where "X" was filled in with the actual number of years that had elapsed.[17] The only difference between treatment and control providers was that

[17] To avoid disturbing providers, Platform O sent out a maximum of two messages to each provider in one week. Specifically, on the first day of each week during the experiment, for provider i, Platform O identified the dates during that week on which provider i uploaded any video exactly one or more years ago. If more than two dates satisfied the criterion, Platform O picked the dates on which the video uploaded exactly one or more years ago had the highest or second highest views (among all videos uploaded in the same week one or more years ago).

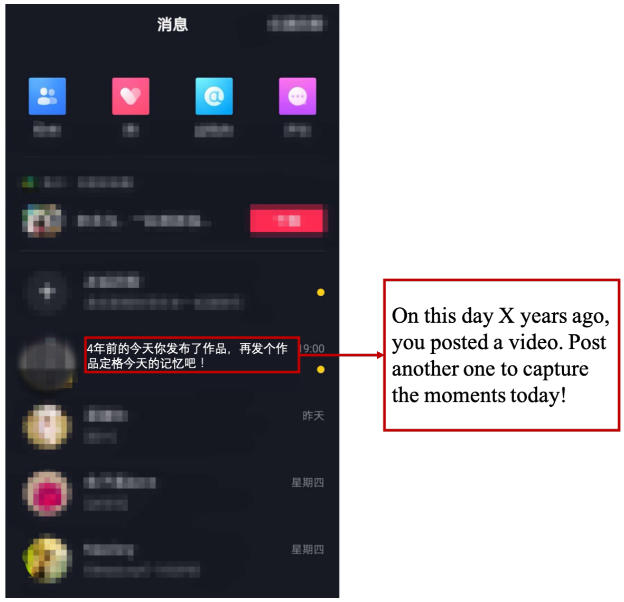


Figure 4 Illustration of a platform-initiated nudge[18]

Platform O actually sent out the aforementioned message to treatment providers on that date, but not to control providers. Therefore, control providers could not receive any platform-initiated nudges. Messages about platform-initiated nudges were displayed in the same place (Figure 4) as social nudges (Figure 1 (b)). If a treatment provider logged onto Platform O after Platform O sent a nudge message, she would see a red number on top of the "message" button indicating the number of unread messages. By clicking the "message" button, the treatment provider would enter the message center and could view the platform-initiated nudge. Similar to the social nudge experiment, treatment providers in the platform-initiated nudge experiment did not receive any notifications about Platform O's nudges if they did not log onto the platform. Thus, the design ensures that treatment providers could not have decided whether to log onto Platform O based on whether they received Platform O's nudges and that Platform O's nudges could only have affected treatment providers if they logged on after Platform O sent them nudges.

We first selected treatment and control providers who satisfied the following criteria: (1) they were qualified to be sent at least one message from Platform O during our experiment; and (2) they logged onto Platform O at least once during our experimental period on or after the day when they first became qualified (so they could see the message from Platform O had they been randomly assigned to the treatment condition). For these providers, we defined "the first reception day" as the day when they first visited Platform O on or after the day when they first became qualified to be sent Platform O's nudge message (regardless of whether or not the providers were actually

[18] Similar to Figure 1, we created Figure 4 by modifying the app interface of a widely-used video sharing platform whose interface is similar to Platform O, in order to protect Platform O's identity.

sent the Platform O's nudges or if they actually viewed the Platform O's nudges). The sample selection criteria and the definition of the first reception day here match our approach in the social nudge experiment (as depicted in Figure 2). Second, among providers who satisfied criteria (1) and (2), we further selected providers who had not uploaded videos for one or more days by the time Platform O sent them a nudge, since providers in the social nudge experiment could only be nudged by neighbors when they had not uploaded videos for one or more days. This further allowed us to create a set of providers that was comparable to the providers involved in the social nudge experiment. Overall, our data included $6,143,326$ treatment and control providers involved in the platform-initiated nudge experiment.

7.2. The Short-term Effects of Platform-Initiated Nudges on Productivity

We first compared the short-term effects on productivity of social nudges versus platform-initiated nudges. Consistent with our analytical strategy for the social nudge experiment (Section 5.1), we examined how receiving a nudge from Platform O affected providers' productivity on the first reception day, and our unit of analysis was each provider on her first reception day (N = $6,143,326$ observations). We used regression specification (1) to predict $\textit{Upload Incidence}_i$ and $\textit{Number of Videos Uploaded}_i$ on the first reception day (as defined in Section 5.1) and report the results in Table 6 Panel A. As shown in Column (1), the platform-initiated nudge treatment lifted the average probability of providers uploading any videos on the first reception day by 0.79 percentage points ($p < 0.0001$), which was a 5.84% increase relative to the average in the control condition. As shown in Column (2), the number of videos uploaded on the first reception day was boosted by 0.0140 standard deviations ($p < 0.0001$), which amounts to a 5.15% increase relative to the average in the control condition.

Since the platform-initiated nudge experiment only involved providers who had uploaded videos in the previous years, we constructed a comparable sample of providers for the social nudge experiment by selecting only providers who had published videos at least one year prior to our social nudge experiment (N = $862,702$ providers). For these providers, we used regression specification (1) to predict $\textit{Upload Incidence}_i$ and $\textit{Number of Videos Uploaded}_i$ on their first reception day. We report the results in Table 6 Panel B. As shown in Column (1), in terms of the absolute effect size, receiving a social nudge boosted $\textit{Upload Incidence}_i$ by 1.51 percentage points, which is substantially larger than the increase of 0.79 percentage points brought by receiving a platform-initiated nudge. In terms of the relative effect size, receiving a social nudge boosted $\textit{Upload Incidence}_i$ by 10.71%, which is almost twice as large as the 5.84% increase brought by receiving a platform-initiated nudge. Column (2) also shows similar patterns: receiving a social nudge boosted $\textit{Number of Video Uploaded}_i$ by 0.0271 standard deviations (or 8.95%), which is considerably larger than the increase of 0.0140

Table 6 Comparison of Social Nudge and Platform-initiated Nudge

Panel A: Short-term Effects of Platform-initiated Nudge on Productivity		
Outcome variable	Upload Incidence	Number of Videos Uploaded
	(1)	(2)
Treatment	0.0079****	0.0140****
	(0.0003)	(0.0008)
Relative effect size	5.84%	5.15%
Observations	6,143,326	6,143,326

Panel B: Short-term Effects of Social Nudge on Productivity		
Outcome variable	Upload Incidence	Number of Videos Uploaded
	(1)	(2)
Treatment	0.0151****	0.0271****
	(0.0008)	(0.0022)
Relative effect size	10.71%	8.95%
Observations	862,702	862,702

Note: Standard errors are reported in the parentheses. *p<0.05; **p<0.01; ***p<0.001; ****p<0.0001

Note: Number of videos uploaded was standardized to have zero mean and unit standard deviation before entering the regressions in Panels A-B Column (2).

Note: The unit of analysis for Panels A and B was a provider on her first reception day. Columns (1)-(2) of Panel A included all providers who satisfied sample selection criteria for the platform-initiated nudge experiment. Columns (1)-(2) of Panel B included the subset of providers in the social nudge experiment who published videos at least one year prior to the social nudge experiment.

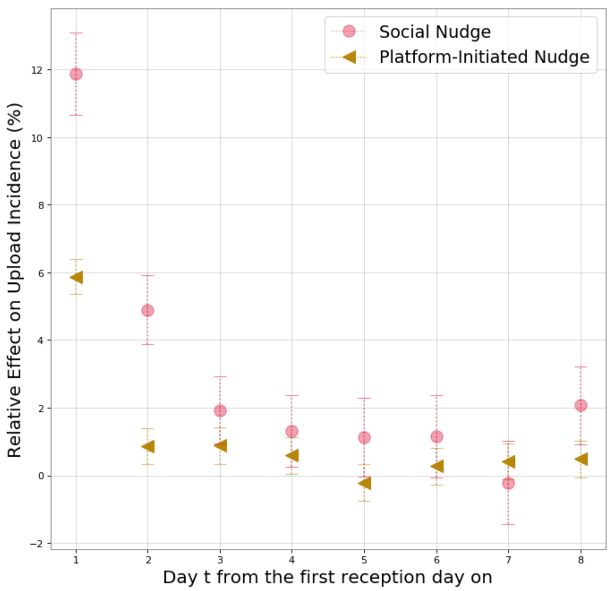
standard deviations (or 5.15%) brought by receiving a platform-initiated nudge. These results suggest that our specific type of social nudge has a stronger short-term effect on productivity than the specific type of platform-initiated nudge under study.

7.3. The Over-Time Effect of Receiving Platform-Initiated Nudges on Productivity

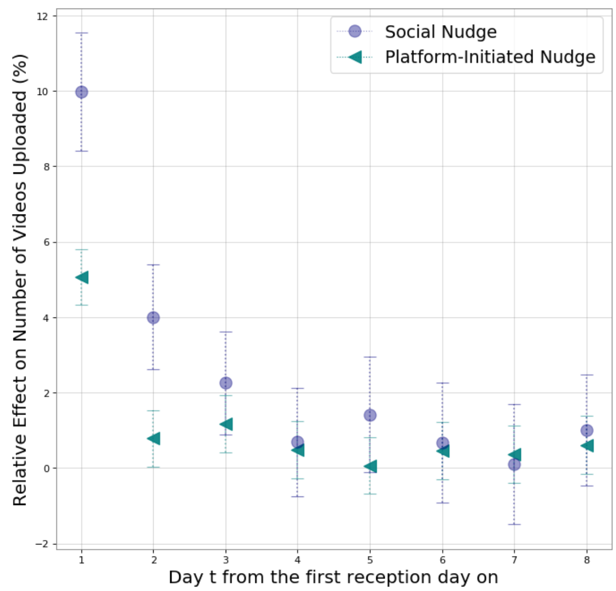
Next we examined the over-time effect of receiving a platform-initiated nudge on productivity, and compared platform-initiated nudges with social nudges in terms of how strong their effects were beyond the first reception day. Since we examined the over-time effect of receiving one social nudge across the seven following days, we also used a seven-day time window here. To examine the over-time effect of receiving *one* platform-initiated nudge, we selected providers who did not satisfy the criteria to receive another nudge from Platform O during the seven days after their first reception day. Our data for the over-time analysis included $4,035,770$ providers (66% of all providers included in the platform-initiated experiment).[19] To compare two kinds of nudges using samples that are as similar as possible, for the social nudge experiment, we used $698,013$ providers

[19] Among these $4,035,770$ providers, the short-term effect of receiving a platform-initiated nudge on *Uploaded Incidence*$_i$ and *Number of Videos Uploaded*$_i$ was 5.87% and 5.07%, respectively, both of which were very similar to the corresponding results for all providers.

who uploaded videos at least one year prior to our social nudge experiment and who were sent only one nudge during our social nudge experiment.



(a) Over-time Effect on Upload Incidence



(b) Over-time Effect on Number of Videos Uploaded

Note: The error bars represent 95% confidence intervals.

Figure 5 Comparing the relative effect size of receiving a platform-initiated nudge versus social nudge over time

We estimated the effect of receiving one platform-initiated nudge or one social nudge on providers' $\textit{Upload Incidence}_{it}$ and $\textit{Number of Videos Uploaded}_{it}$ on day $1, 2, \cdot, 8$ from the first reception day on (where $t = 1$ refers to the first reception day itself), using the same analytical approach described in Section 6.2. We plotted the relative effect sizes and the corresponding 95% confidence intervals in Figure 5.[20]

As shown in 5 (a), the effect of receiving a platform-initiated nudge on $\textit{Upload Incidence}_{it}$ was largest on the first reception day, decreased with time, and was generally smaller than that of receiving a social nudge. In particular, on the first day after the first reception day (i.e., $t = 2$), a social nudge increased providers' probability of uploading any video by 4.89% ($p < 0.001$), more than five times as large as the increase of 0.87% ($p < 0.0001$) from a platform-initiated nudge; on the second day after the first reception day (i.e., $t = 3$), a social nudge increased providers' probability of uploading any video by 1.92% ($p < 0.0001$), more than two times as large as the increase of 0.88% ($p < 0.001$) brought by a platform-initiated nudge. Similarly, Figure 5 (b) indicates that the effect of receiving a platform-initiated nudge on $\textit{Number of Videos Uploaded}_{it}$ decreased with time

[20] We report the absolute effect sizes and the corresponding 95% confidence intervals in Online Appendix B.

and was generally below that of receiving a social nudge. In particular, on the first day following the first reception day (i.e., $t = 2$), a social nudge increased the number of uploaded videos by 4.01% ($p < 0.0001$), more than five times as large as the increase of 0.79% ($p < 0.001$) from a platform-initiated nudge; on the second day following the first reception day (i.e., $t = 3$), a social nudge increased the number of uploaded videos by 2.26% ($p < 0.01$), almost two times as large as the increase of 1.17% ($p < 0.01$) brought by a platform-initiated nudge.

8. General Discussion

In two field experiments (the main experiment and a replication experiment) on a large online platform, we tested the effects of social nudges on productivity and consumption. We present a number of key findings. First, in the short term, we causally demonstrate that social nudges motivate providers to upload more videos, which further boosts video consumption. In particular, providers who were historically less recognized have a stronger positive response to social nudges than these who were historically more recognized. Second, our long-term analysis suggests that social nudges can expand supply (rather than simply shifting future supply to the present) and that the motivating effect of receiving *one* social nudge, though declining over time, lasts several days. Moreover, we provide suggestive evidence that the specific social nudge we studied can generate a stronger effect on productivity than the specific type of platform-initiated nudges we studied, both in the short term and the long term. Overall, our documented effects are practically meaningful in two ways: from a supply perspective, our social nudge treatment increased the total number of videos supplied by 9.05%, and it also diversified the pool of content providers by increasing the daily number of active providers by 10.27%; from a demand perspective, our treatment boosted video views by 5.56%, contributing positively to the platform's profits as well.

Our research offers a number of practical implications for online platforms beyond our setting. First, our findings suggest that social nudges can be a cost-effective intervention for lifting productivity on the supply side and consequently increasing consumption on the demand side. Compared to financial incentives, social nudges require minimal costs on the platform's end and can yield a higher return on investment. Moreover, since our findings suggest that social nudges may particularly inspire historically less recognized workers to produce more, social nudges can contribute to platforms' long-term diversity and vitality. These workers have the potential to grow their viewership as they offer more content/products/services, but may need more opportunities to grow compared to providers with a high historical recognition who may already be in their prime. By urging workers with less historical recognition to produce more, social nudges may facilitate their growth and thus benefit platforms' sustainable development.

Second, our research highlights the importance for platform managers to leverage the influence of co-users and, in particular, neighbors. Online platforms have little authority over their users' actions (such as when and how much they supply), rendering it challenging for platforms to implement heavy-handed interventions to achieve desired outcomes. However, one of online platforms' main advantages lies in connecting users and facilitating transactions or relationships between users. Our research suggests that platforms can guide co-users to make an effort to improve the overall user engagement on platforms, such as by offering mechanisms via which co-users motivate each other to produce or consume more content/products/services.

Third, the success of our information-based intervention should encourage online platforms to take advantage of their advanced information technologies and explore other information-based interventions. A social nudge conveys the sender's interest in the recipient's future work. Following this logic, platforms can disseminate information to make workers feel more valued and more optimistic about the potential success of their future work, which may enhance workers' supply of content, products, or services. As an idea for online platforms like Platform O, if providers have not been active for a while, platforms can inform them of how many users have recently visited their profile pages and viewed their past videos. As an example from a service platform, Uber encourages riders to give drivers badges or to write a personal thank-you note, and presents such information to drivers and other riders.[21]

Several limitations about our research open up interesting future research directions. First, the type of social nudge we examined is simple, private, and subtle. Specifically, our social nudge message was standardized across users, contained very simple content, and did not leverage additional psychological principles. Messages about social nudges were displayed privately (i.e., only visible to recipients) in a relatively hidden place (as described in Section 4). In fact, as more messages arrived to providers' message center, the message about a previous social nudge was pushed down and was not visible on the front page of the message center. Using such a light-touch, bare-bones social nudge allowed us to provide a clean test of the effect of being nudged. Nevertheless, future research could examine how to design social nudges to produce stronger and more long-lasting effects—for example, by incorporating persuasion techniques and additional psychological insights into nudge messages, allowing senders to write personalized messages, or publicly displaying social nudge messages that never fade away. Future research could also explore how the effects of social nudges vary based on the characteristics of the sender-recipient pair and guide senders to nudge recipients who they are most likely to influence.

Second, we could not causally study the effects of repeatedly receiving social nudges since the number of social nudges sent to each provider was not exogenous. Future research could randomly assign people to receive varying numbers of nudges and causally estimate how the effects of social

[21] https://www.uber.com/hk/zh-hk/ride/how-it-works/driver-compliments/

nudges change with the number of nudges received. Third, we compared social nudges and platform-initiated nudges using observations from two separate experiments, and only analyzed one specific type of platform-initiated nudge, so we could only provide preliminary evidence for how social nudges fare in comparison. To better understand how nudges from neighbors compare to nudges from platforms, future research should randomly assign users to receive different types of social nudges and platform-initiated nudges. Finally, we focused primarily on examining how receiving a social nudge changed providers' behavior. Future research could explore how knowing the effect of one's nudge alters a sender's willingness to continue nudging others and, more broadly, how to encourage co-users to send social nudges and exert influence.

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Online Appendix

Appendix A: The Second Social Nudge Experiment as a Replication

We conducted another experiment to replicate the main effect of social nudges on productivity that we observed in the first field experiment (Section 5.1). The replication experiment lasted from 5pm on September 14, 2018 to the end of September 20, 2018. It lasted longer than the main experiment and targeted a non-overlapping but much smaller sample of providers than the main experiment.[22] Providers targeted by the replication experiment were randomly assigned into either the treatment condition or the control condition. Similar to our main experiment (Section 4), our data for the replication experiment included providers who satisfied two criteria: (1) at least one of their neighbors sent them a social nudge during the experiment and (2) they logged onto Platform O at least once during the experiment on or after the day when the first social nudge for them was sent. Our data consisted of 988,985 qualified providers, among whom 493,771 were in the treatment condition and 495,214 were in the control condition.

Table 7 Short-term Effects of Social Nudges on Productivity (Replication)

Outcome variable	Upload Incidence	Number of Videos Uploaded
	(1)	(2)
Treatment	0.0139****	0.0253****
	(0.0007)	(0.0020)
Relative effect size	11.05%	9.16%
Observations	988,985	988,985

Note: Standard errors are reported in the parentheses. *p<0.05; **p<0.01; ***p<0.001; ****p<0.0001

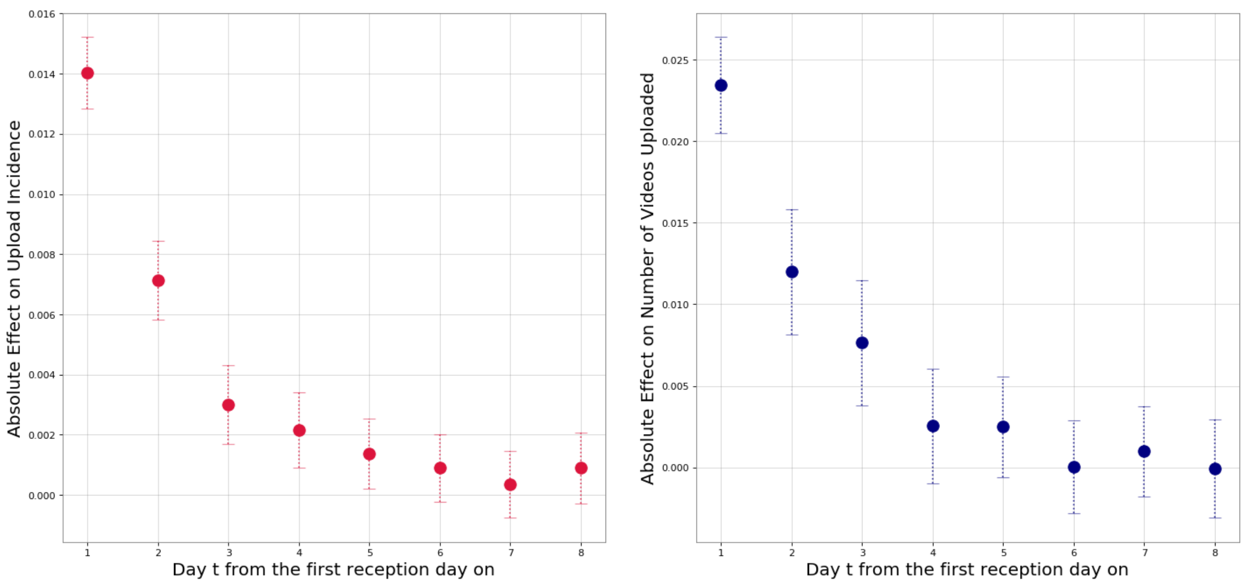
Note: Number of videos uploaded was standardized to have zero mean and unit standard deviation before entering the regression in Column (2). The unit of analysis for both columns was a provider on her first reception day, and both columns included all providers in the replication experiment who satisfied the sample selection criteria.

We used regression specification (1) to predict $\textit{Upload Incidence}_i$ and $\textit{Number of Videos Uploaded}_i$. As shown in Column (1) of Table 7 Panel A, receiving a social nudge lifted treatment providers' average probability of uploading any video on the first reception day by 1.39 percentage points (or 11.05%; $p < 0.0001$), compared to control providers. This effect size is comparable to the effect size observed in the main experiment where treatment providers' average probability of uploading any video was lifted by 10.27% ($p < 0.0001$) relative to control providers. Also, as shown in Column (2) of Table 7 Panel A, receiving a social nudge increased the number of videos treatment providers uploaded on the first reception day by 0.0253 standard deviations (or 9.16%; $p < 0.0001$), compared to control providers. This effect size is comparable to the effect size of 9.06% ($p < 0.0001$) observed in the main experiment. To sum up, we replicated that social nudges could immediately increase providers' productivity on the first reception day.

[22] We first randomly sampled a portion of providers to be included in the main experiment. Then among the remaining providers, we randomly sampled a smaller portion of providers to be involved in the replication experiment.

Appendix B: The Over-time Absolute Effect of Receiving a Social Nudge or a Platform-initiated Nudge on Productivity

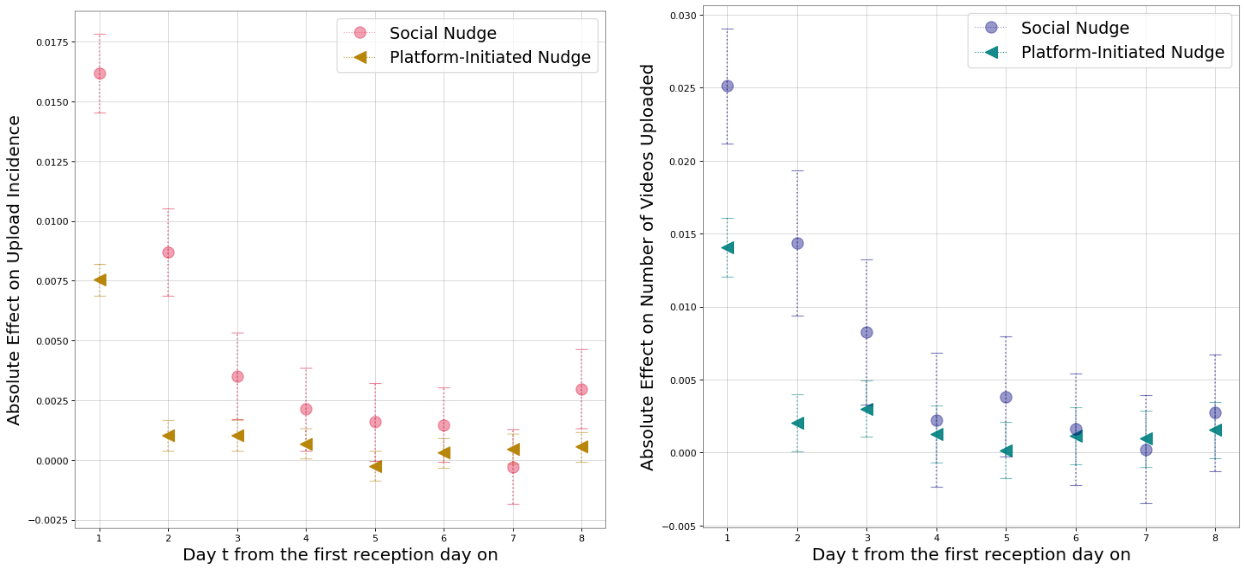
To supplement Section 6.2, in Figure 6, we plotted day by day the absolute effect size (i.e., the coefficient on treatment for a given outcome variable on a given day) of receiving a single social nudge on *Upload Incidence*$_{it}$ and *Number of Videos Uploaded*$_{it}$, along with the corresponding 95% confidence interval (i.e., the upper and lower bounds of the coefficient on treatment). For *Upload Incidence*$_{it}$, the figure shows changes in the raw probability of uploading something on a day (e.g., 0.01 representing a one-percentage-point increase). For *Number of Videos Uploaded*$_{it}$, the figure shows the magnitude of changes in the unit of standard deviation (e.g., 0.01 representing an increase by 0.01 standard deviations). We focused on providers who were only sent one social nudge during our experiment, as explained in Section 6.2. As expected, the results show consistent patterns as the patterns of relative effect sizes reported in Section 6.2.



(a) Over-time Effect on Upload Incidence (b) Over-time Effect on Number of Videos Uploaded

Figure 6 Illustration of how the absolute effect of receiving a social nudge varied over time

To supplement Section 7.3, in Figure 7, we plotted day by day the absolute effect size and the corresponding 95% confidence interval of receiving a single social nudge versus a platform-initiated nudge on *Upload Incidence*$_{it}$ and *Number of Videos Uploaded*$_{it}$. We used the same samples as described in Section 7.3. As expected, the results show consistent patterns as the patterns of relative effect sizes reported in Section 7.3.



(a) Over-time Effect on Upload Incidence (b) Over-time Effect on Number of Videos Uploaded

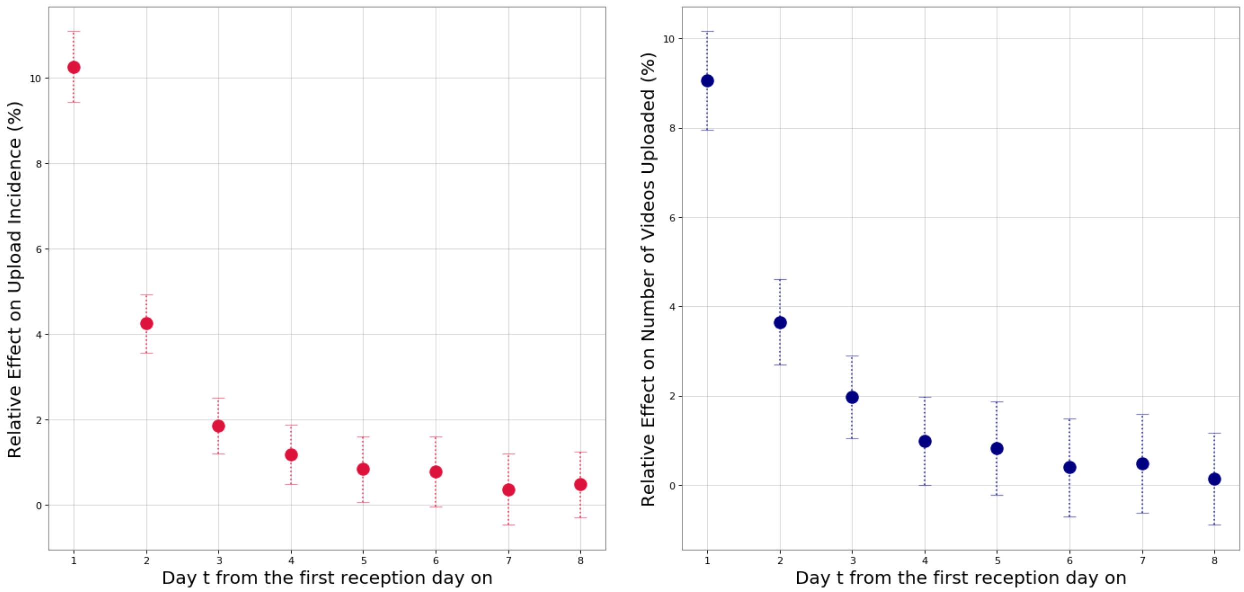
Figure 7 Comparing the absolute effect of receiving a platform-initiated nudge versus social nudge over time

Appendix C: The Over-time Effect of Receiving Social Nudges on Productivity

In Section 6.2, we focused on providers who were sent only one social nudge during our experiment. We note that this sample is not perfect for a causal estimation. Specifically, since providers could not be nudged within one day after they uploaded any new video and social nudges caused treatment providers to be more likely to upload videos (as we have shown in Section 5.1), treatment providers may be less likely to receive additional nudges during our experiment, which may render treatment and control providers who were sent only one social nudge during the experiment not perfectly comparable for an ideal causal estimation.

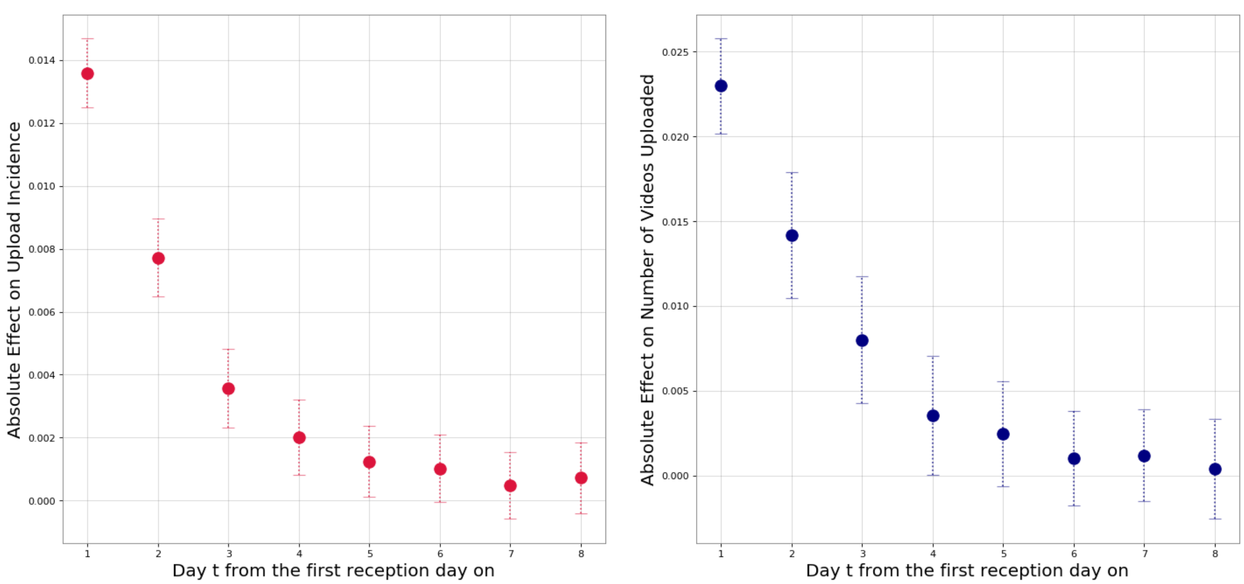
As a robustness check, we conducted the same analysis for the full sample of providers. For each day t from the first reception day on (t $= 1, 2, \cdots, 8$ and $t = 1$ refers to the first reception day itself), we separately predicted $Upload\ Incidence_{it}$ and $Number\ of\ Videos\ Uploaded_{it}$ using regression specification (1). Each day had $1,526,574$ observations corresponding to the number of providers in our full sample. We plotted the relative effect sizes and the corresponding 95% confidence intervals in Figure 8. As shown in Figure 8 (a), the relative effect of receiving social nudges on $Upload\ Incidence_{it}$ was largest on the first reception day (i.e., $t = 1$), decreased as time elapsed, but was always in the positive direction. In fact, treatment providers' likelihood of uploading any video on the first day after the first reception day (i.e., $t = 2$) was higher than that of control providers by 4.26% ($p < 0.0001$), and the positive effect remained statistically significant up to four days after the first reception day (i.e., from $t = 1$ to $t = 5$). As shown Figure 8 (b), the relative effect of receiving social nudges on $Number\ of\ Videos\ Uploaded_{it}$ decreased with t and was positive and significant for four days from the first reception day on (i.e., from $t = 1$ to $t = 4$). The absolute effect sizes and the corresponding 95% confidence intervals were plotted in Figure 9 as a supplement and showed the same patterns.

The results involving all providers are basically the same as the results involving only providers who were sent one social nudge during our main experiment (as reported in Section 6.2). The analysis based on the full sample relied on perfectly comparable treatment and control providers, but may not purely reflect the over-time effect of receiving *a single* social nudge since 18% providers in the full sample had multiple social nudges sent to them during the main experiment. For example, if two social nudges were sent to a provider on her first reception day, then this analysis would have picked up the effect of receiving two social nudges, rather than a single nudge, on this provider. As another example, if a provider's first reception day was on September 12, 2018 and a new social nudge was sent to her on September 14, 2018, then this analysis could not cleanly reflect the residual effect of receiving the initial social nudge two days after her first reception day (i.e., on September 14). Therefore, in the paper, we focused on providers with only one social nudge during our main experiment to illustrate the over-time effect of receiving *one* social nudge. We believe it is the best possible approach and are confident in our results since our robustness check revealed a very similar pattern.



(a) Over-time Effect on Upload Incidence (b) Over-time Effect on Number of Videos Uploaded

Figure 8 Illustration of how the relative effect size of receiving social nudges varied over time



(a) Over-time Effect on Upload Incidence (b) Over-time Effect on Number of Videos Uploaded

Figure 9 Illustration of how the absolute effect of receiving social nudges varied over time